THURSDAY, JUNE 5, 1913.

DYNAMICS OF GOLF.

The Soul of Golf. By P. A. Vaile. Pp. xiii + 356. (London: Macmillan and Co., Ltd., 1912.) Price 6*s.* net.

THIS is a breezy, well-written book, full of valuable hints to the ambitious golfer. Much of the instruction takes the form of attack upon the writings of other exponents, and this makes lively reading. With a great deal of the criticism most golfers who have tried to formulate the principles of the game will agree. But it is not human to be perfect, and Mr. Vaile does not escape falling into pitfalls himself, especially when dealing with the dynamical aspect of things. To the November number of *The Fortnightly Review* he contributed an article on the dynamics of golf, in which he seems to regard himself as a supreme authority. In one respect this article is an improvement on the book, for he accepts in its simplicity Newton's explanation of the swerve of the spinning ball advancing in air, and in one paragraph gives quite a good account of this phenomenon, very much as Tait did years ago. Unfortunately he obscures the explanation in a later paragraph when he says that it is "the continual friction on the lower portion of the ball which gradually forces it up."

Mr. Vaile is merciless in his exposure of looseness of language. Yet in his discussion of the value of furrows cut on the face of iron clubs (a method, by the way, first adopted by Tait), he writes that "marking is frequently done by great deep lines, and, particularly in the mashie, nearly always by lines which run from heel to toe. Now in the great majority of mashie shots, when one is putting on cut one requires lines running in an exactly opposite direction"—that is, if English means anything, from toe to heel! Mr. Vaile, of course, does not mean that, any more than Sir J. J. Thomson means that the golf-ball is spinning like a sleeping top when he says that slicing and pulling are due to rotation about a vertical axis. Sir J. J. Thomson does not say that this is the only spin that exists; but it is the component of spin about the vertical axis which has to do with the phenomena of slicing and pulling. When the ball is rotating about an axis not truly horizontal, there must be a component about a vertical axis. This component, regarded from above, will be either right-handed or left-handed, according to circumstances, producing respectively slicing or pulling. This, obviously, is the meaning to be attached to Sir J. J. Thomson's words. In his article Mr. Vaile says: "Prof. Thomson must now realise that if the axis of spin was the same in each ball"—that is, each sliced or pulled ball—"their conduct on landing would be similar." Can Mr. Vaile not imagine that though the axis of spin may be the same, the direction of rotation about the axis may be either the one way or the other?

Mr. Vaile is to be congratulated upon the clear way in which he describes the conditions under which the so-called "push-shot" with cleek or iron is obtained. But the main feature of this stroke was well understood on the golf-links of St. Andrews and Musselburgh long before the days of Varden or Vaile. The divots of turf which were removed by the club after it hit the ball proved incontestably that this particular stroke of club on ball occurred during the descent of the club. In his article on long driving in *The Badminton Magazine* of March, 1896, the late Prof. Tait shows clearly how underspin is produced. In his figures on p. 370, Tait represents the velocity of the clubhead at impact by the line *AB*, and remarks that "*AB* may be made to take any direction we please—*i.e.* the clubhead may be represented as moving in any direction whatever; but it *is quite sufficient for our purpose*[1] to treat it as moving horizontally." Mr. Vaile regards this sentence as meaning that Tait considered every well-driven ball as being projected by a horizontal blow; for he deliberately says that a "fundamental error" of the late Prof. Tait consisted "in regarding the blow of a golf-club as being a force directed in a line parallel with the horizon." This is really very bad. Yet there is a worse case of unpardonable carelessness in Mr. Vaile's reading of the *Badminton* article.

In a footnote on p. 380 of this article Tait, with the honesty of the real investigator, points out the difficulties under which his "laboratory experiments" on the *velocity of projection* of a golf-ball were made, and Mr. Vaile quotes this footnote as if it referred to a totally different experiment described on p. 381. The latter experiment, with one end of a long, untwisted tape "tied to the ball and the other to the ground," was clearly not made in the laboratory at all. The ball was driven into a stiff clay face, not into the 10-inch disc of clay spoken of in the footnote; and the difficulties of aim did not enter into the experiment with the tape. Tait always found the tape twisted in such a way as to show underspin. Mr. Vaile will not believe it. He should therefore try the experiment himself instead of criticising what he has not taken the trouble to understand aright. By this experiment Tait proved to the simplest intellect that underspin was invariably present in a well-struck ball. But before the experiment was thought of Tait had proved to the mind capable of understanding the dynamics of golf that it is not possible to obtain the *distance* of flight and the *time* of flight by balls projected with *permissible* velocities at the *small angles of elevation* which characterise well-driven balls, unless an uplifting force comes into play.

---

[1] The italics are ours.

Mr. Vaile is neither fair to Tait nor true to fact. In his chapter x. he says, quite untruly—for the dates can easily be given[2]—that Tait did not take underspin into consideration until after a great drive by which his son Freddie proved that the father's calculations were wrong. He then proceeds to point out what he thinks are errors in Tait's article in *The Badminton Magazine,* which has been already referred to. For example, Tait's remark that "the existence of rotation is manifested at once by the strange effects it produces on the curvature of the path" is branded "as incorrect from a scientific point of view," and "also badly stated." What follows on p. 225 of "The Soul of Golf" shows that our author has not grasped the scientific significance of the problem. He says that it is well known to all golfers that the spin begins to work as the velocity of the ball decreases, and then makes the astounding statement that "it is incorrect to refer to the strange effects it (rotation) produces on the curvature of the path, for it is the rotation itself which produces the curvature."

If this means anything other than Tait meant, it means that the parabola described by a projectile *in vacuo* has no curvature, nor has the path described in air by a ball devoid of rotation. Gravity apparently is not in it! Mr. Vaile fails to see that Tait is comparing the path of a rotating golf-ball with the path it would have had if no rotation had existed. The real dynamic truth is that the underspin begins to work from the very beginning of the drive. The curvature of the path is influenced *from the start,* as witness the upward concavity of the wind-cheater. Not only so, but the underspin exerts its greatest influence at the beginning of the drive when the spin and the velocity are both at their greatest. When Mr. Vaile asserts that it is in the second part of the trajectory that the back-spin is exerting its greatest influence, he is confusing what the eye seems to see with what has been really taking place. The ordinary golfer, indeed, is quite ignorant of what any particular path would have been had there been no spin; also he is badly placed to see the real form of the path, and the initial velocity is too great for him to follow the early details of the flight. Tait's calculated curves bring out the form of the wind-cheater beautifully (see NATURE, June 29, 1893). These calculations are based on the combined effect of gravity and the upward force due to coexistence of translation and spin. Both these motions are present from the beginning, and must dynamically assert themselves from the start. To speak of pace, when there is enough of it, as beating spin, is nonsense. Spin alone has no lifting or swerving power. It must coexist with translational velocity; and the lifting power increases with the amounts of both.[3] The particular instant at which the eye recognises the accumulated effect of the upward lift proves nothing as to the manner in which this lifting force has gone through its successive values. The idea that the spin asserts itself only after the ball has travelled a considerable part of the trajectory is dynamically grotesque and hopelessly erroneous.

The flight of the golf-ball is not a problem which can be solved by intuition; and the man who has not mastered Tait's papers on the path of a rotating spherical projectile is not in a position to criticise Tait's conclusions. If Mr. Vaile will take the trouble to turn up the first of these papers he will find pictured a theoretical curve the gradually increasing curvature of which as the linear speed of the projectile falls off will reveal to him all the essential characteristics of the path of the sliced ball. He may then possibly understand the truth which Tait was wont to impress upon his students year by year, that the direct evidence of our senses is frequently misleading unless controlled and corrected by reason.

There are several other statements in Mr. Vaile's account of the flight of the golf-ball which are scientifically unsound. Thus, on p. 244 he says that the club

> "is nearly always moving either upwards or downwards in a curve at the moment it strikes the ball, so that it stands to reason, especially when the club-face is travelling upwards, which is what it does in the great majority of cases, that the blow is never delivered horizontally, but is always struck more or less upward through the ball's centre of mass."

This is deliciously loose, for it means grammatically that when the club-face is travelling either upwards or downwards it strikes the ball upward through the centre of mass. Of course, Mr. Vaile does not mean to say so. What he seems to mean to assert is that in the *great majority of cases* the impulse is through the centre of mass of the ball. If that were so, there could be no underspin, nor, indeed, any spin at all. The great majority of strokes would be unaccompanied by the phenomena of slicing or pulling or rising above the natural gravitational path. The dynamical condition for the production of underspin is clearly pointed out by Tait in his various articles. The resultant blow must be delivered so as to act in a line which passes beneath the centre of mass. For this purpose the blow need not be horizontal; but a horizontal stroke with a lofted club will produce underspin if the club-face hits the ball below the height of the centre. In this sense loft is an important factor in the production of underspin. In all cases, whether the ball is hit with a downward or a practically horizontal stroke, the production of underspin depends on the existence of a pronounced tangential component of impulse, and this requires that the direction of the blow must be inclined to the face of the club.

---

[2] See, for example, "A Golf Myth: Prof. Tait's Alleged Error," in *Golf Illustrated,* January 1, 1909; or in my "Life and Scientific Work of P. G. Tait," pp. 26–28.

[3] See, for example, Lord Rayleigh, "On the Irregular Flight of a Tennis Ball," *Messenger of Mathematics,* 1877; "Scientific Papers," vol. i.

Mr. Vaile expresses the same idea when he speaks of the ball being hit with a glancing blow. In his explanation of the manner in which the wind-cheater is produced he is indeed quite sound; and it is a matter of regret that a book so admirable in many respects should be marred, not only by faulty dynamics, but by an inability to follow the dynamical reasoning of a master like Tait. Mr. Vaile sneers at the mathematician and physicist as having gone on utterly fallacious lines. He misquotes, and when he quotes aright he not unfrequently misunderstands. Nevertheless, on the more practical aspects of the game, when he is speaking for himself, and not piling up adjectives of denunciation against the mathematician, Mr. Vaile has done no small service in removing some of the clouds of mystery with which popular writers have obscured the soul of golf.

C. G. KNOTT.

THE AGE OF THE EARTH.

The Age of the Earth. By A. Holmes. Pp. xii + 196. (London and New York: Harper and Brothers, 1913.) Price 2*s*. 6*d*. net.

THE question "For how long has it been possible for organic beings to have lived on the earth?" must always be one of supreme interest; and it is good to find a book which states shortly but quite clearly how far we have proceeded towards an answer.

No great weight ought ever to have been given to the argument from the lengthening of the day, because it assumes that the ratio of the polar and equatorial diameters of the earth now is the same as when the earth ceased to be liquid; in spite of the fact that great forces are acting tending to change this ratio. Kelvin's argument from the temperature gradient downwards in the earth's crust ceased to be of value when it was shown that greater conductivity in the interior led to an enormously larger answer; for whether such greater conductivity is or is not probable, it could not be said to be impossible. We are sorry that Mr. Holmes should refer to the work of Mr. Clarence King as if it affected the question. He, following Kelvin, assumed that there could be no greater conductivity inside the earth than in the crust. But all earth-cooling arguments have been set aside by Mr. Strutt's measurement of radium in rocks, and they are now of historical interest only.

Three of the old arguments still hold the field —one from the sun's energy, a second from the amount of salt in the oceans, and the third from observations of rates of erosion and deposit of sediment. To these a fourth is now being added which is likely to have great weight in settling the matter—the increase in the proportion of lead to uranium in rocks as time goes on. Mr. Holmes has himself devoted much time to the laboratory study of radio-active minerals, and of the creation of lead and helium from uranium; in the present state of our knowledge we can suggest no modification of his figures. He ought, however, we think, to be prepared to accept a less age for the earliest sedimentary rocks than 1300 million years.

Consideration of the amount of sodium in the ocean gives less than a quarter of this age, as does also the consideration of the accumulation of carbonate of lime. These two methods of study are on a much less certain basis than the calculation from the rate of accumulation of sediment, which, however, gives about the same age. With this last method Mr. Holmes, as a geologist, is very familiar. The suggestion that erosion used to take place more slowly because all continents were smaller and lower in level than now, would lead to a better agreement between the two methods which he favours. If the average slopes were 60 per cent. of what they are now, we are led to multiply the age by four. On the whole, we feel with Mr. Holmes that the question is in a fair way towards settlement, but, unlike him, we still see a difficulty due to the age of the sun. A person who has not made the calculation will scarcely believe in the liberality with which Kelvin treated his opponents in regard to the argument based on the sun's heat. Assuming that the whole mass of the sun was once scattered through space, and by mere gravitation the stuff came together as we now have it, and considering that it is denser in its central part, the total amount of energy given out as heat cannot be much greater than 25 million